

in terms, the machine was ever put to work, although it is said that M. Amonton made many experiments to convince the Academy of the practicability of his invention. M. Amonton proposed to have a metallic wheel revolving on a horizontal axis; the outer rim of the wheel was to be divided into a number of separate air cells, each of which had a channel so as to communicate with other cells, water-cells, arranged round the wheel nearer to the centre than the air-cells; the air-cells as they passed over a fire were to be heated, and the air was to drive this water up to one side of the wheel, so as to keep that side always loaded, and thus give the wheel a tendency to revolve. The cells after leaving the neighbourhood of the fire were to be cooled by passing through water to re-contract the air ready for the next operation.

No. 1,940, which is before me, is a model of Stirling's hot-air engine, but time does not remain to describe it.

Besides hot-air engines, we have had engines working by the explosion of gunpowder, and others working by the explosion of gases. No. 1,945 is Langen and Crossley's gas engine, from which I believe extremely excellent results have been obtained.

I will now ask you to look at a tabular statement which shows the consumption of fuel in some agricultural engines, when under trial, expressed in pounds per horse-power per hour, and also in millions of pounds raised one foot high by the consumption of 1 cwt. of coals. I told you how excellent were the results at which our agricultural engineers had arrived; you will see that one of those machines, working with 80 lbs. steam, and of course without condensation, has developed, not a gross indicated horse-power, but an actual dynamometrical horse-power, for 2 79 lbs. of coal per horse per hour, giving a duty of as much as 79½ millions. This high result was obtained by the excellence of the boiler and of the combustion, as well as by that of the engine. If you look at the column of evaporation you will find that as much as 11 83 lbs. of water were converted from the temperature of the boiling point into steam by the combustion of 1 lb. of coal; this was due, not to the merits of the boiler alone, but to the extraordinary ability of the stoker, and to the care and labour bestowed, a care and labour far too expensive to be employed in practice. But should not we engineers endeavour to ascertain whether we cannot by mechanical means, practically, with certainty and cheapness, procure an accuracy of combustion as great, or even greater than that which can be got by the almost superhuman attention of a highly-trained man, who at the end of four hours of such work is utterly exhausted? Many forms of fire-feeders have been attempted and used with more or less success, but I cannot help thinking that in order to obtain the accurate proportioning of air and fuel, by which alone we can get efficient and economical combustion, we shall have to turn our attention in the direction of dealing with the fuel in a comminuted state, either by converting it into gas, as is done by our president, Dr. Siemens, by availing ourselves of liquid fuel, or by employing the process of Mr. Crompton, and making the fuel into an impalpable powder, that may be driven into the furnace by the air which is there to consume it.

By these, and by other means, we may hope to improve combustion. By strict attention to the proportioning of the parts of the boiler we may hope to make the best use of this improved combustion. By higher initial pressure, by greater expansion, and by the general employment of condensation, wherever practicable (and by the use of the evaporative condenser there are very few cases in which it is not practicable), we may trust that the steam-engine, even on its present principle, will be rendered more economical than it has ever yet been, and may give us more than that one-eighth or one-ninth of the total force residing in the fuel which now alone we get under the very best and most exceptional conditions. A large loss, however, must with steam-engines, as we now know them, always be incurred. We cannot hope to deal with initial pressures and temperatures corresponding with steam of a density equal to that of water, nor to carry expansion down to the point where ice would be formed in the condenser. But wonderful as the steam-engine is, worthy as it was and is of Belidor's eulogium (which I read to you), we know it is not the only heat motor, and we are aware that there are other forms of such motors which, theoretically at all events, promise higher results.

By improvements in the existing steam-engine, by the invention and development of other heat motors, by the employment of the power of water and of wind, either as principal motors or as auxiliaries, we may look to further progress in the machines—the subject of my address—"Prime Movers."

I have brought before you, of necessity hastily, and therefore (and also on account of my own incapacity for the task) imper-

fectly, the leading improvements which have been made in prime movers from the date of the water-wheels of Vitruvius to the best-devised steam-engines of our own day. These improvements have been effected by men like Papin, Savery, Newcomen, Watt, Symington, Stephenson, and others, who were not mere makers of engines, but were men full of an ardent love of their noble profession, who followed it because of the irresistible attraction it possessed for them; followed it from their boyhood to their grave, and in that very following found their great reward. These men undoubtedly possessed that combination of science and practice, which combination, Dr. Tyndall has told us, is necessary if either science or practice is to continue to live; for, to use his expressive language, without this combination they both die—die of atrophy; the one becomes a ghost, the other a corpse.

We have every reason to believe that this combination will rapidly become even more fully developed, not only in the engineers of the present day, but in those of the next and of succeeding generations, and to such men as these we may trustfully leave the continued improvements of prime movers, resting content with the knowledge that a more general application of these machines must of necessity follow such improvements, and that the day will soon dawn when in no civilised country will there continue to be the temptation to employ intelligent humanity in the brutal labour of the turnspit, or of the criminal on the treadmill.

OCEAN CIRCULATION¹

THE present theories with regard to ocean circulation do not appear to account for many of the phenomena with which we are acquainted; and my object in this paper is to state very briefly my own opinions, with a view to provoking discussion, and, in this way, to forward the knowledge of a very difficult but interesting subject. I believe that there are at the present moment two rival doctrines, viz.:—

1. One which attributes all currents to the influence of the winds.

2. Another which attributes all ocean currents to gravitation.

I entirely disagree with the first doctrine, and shall address my remarks to the second. I quite think that ocean circulation is the result of gravitation, but, contrary to what I believe to be the present opinion, I hold that the cold feeding streams flow in a wave from the surface of the Polar oceans, and not from the bottom.

The points that I wish particularly to suggest for consideration are as follows:—

1. That all ocean currents run from a higher to a lower level.

2.² That the upward pressure produced in the equatorial regions by the constant inflow, at the bottom, of water from the Polar regions owing its high specific gravity to its contraction from cold; and, *vice versa*, the constant inflow at the bottom of the Polar regions, of water flowing from the equatorial regions and owing its high specific gravity to its salinity, must, these streams flowing from a higher to a lower level, tend to elevate the lighter surface-water and drift it down a slightly inclined plane as a surface-current.

3. That the primary cause of the origin of all ocean currents is the change in the specific gravity of sea-water from one of the following causes, viz.:

(a) Evaporation; the vapour arising from the surface being fresh, and leaving its saline constituents behind it.

(b) The excess of precipitation over evaporation, particularly in the Polar seas, which by admixture with the surface-water increases its freshness.

(c) The expansion of surface-water through heat.

(d) The contraction of sea-water through cold.

It is generally admitted that currents of both air and water flowing from the equator to the poles having an excess of easterly momentum due to the velocity of rotation of the earth's surface in low latitudes as compared with the lesser velocity in high latitudes,³ must outstrip the earth's motion, and consequently

¹ More particularly with reference to the North Atlantic Ocean, being an abstract of a paper read to the Caterham Literary Society in March last.

² I hold it to be impossible that you can have any such thing as an ocean level unless the different strata or layers of water from the equator to the poles are not only isometrical and isothermal, but are also of equal specific gravity; whereas the known ranges of variation of both temperature, salinity, and depth of different strata of sea-water vary much in different places and in different oceans. There is a constant disturbance of equilibrium, and the constant effort to restore and equalise it produces the currents.

³ The rotatory velocity of the earth's surface being about 1,440 feet per second at the equator, 720 feet per second in 60° of latitude, and decreasing to zero at the poles.

flow in an easterly direction; and, on the other hand, currents of both air and water flowing from the poles towards the equator must for the same reason lag behind, and consequently appear to flow in a westerly direction. From this I argue that the cold currents from the Arctic regions to the equator do hug the western shores, and therefore cannot possibly supply the cold streams on the eastern side of the North Atlantic Ocean, south, at all even's, of 50° of lat.; but that the supply must come from the Antarctic Ocean; and, on the other hand, from the same cause, that the cold water on the eastern side of the South Atlantic Ocean is water from the Arctic Ocean which underflows the equatorial stream, and as it approaches the African coast, has a portion of its stream thrown upwards towards the surface, which accounts for the surface-water of the equatorial stream near this coast being some degrees colder than that of the Guinea current to the northward of it.

I need not say that every gallon of water that flows into the North Atlantic from the South Atlantic Ocean must be returned to it in some way, either by a surface or an under-current; and I think I may safely argue that there are no surface-currents sufficient to account for the return of the volume of Antarctic water, and that, therefore, a large portion of the water returned must be from the Arctic basin, and must flow in the manner which I have previously indicated.

The surface-water in these warm regions is lifted by the inflow below it of colder and therefore heavier water from the two polar seas, it then flows off as a surface-current, and the portion of it flowing towards the north pole is deflected by the constant easterly trade winds and obliged to flow westward along the north coast of South America; a large portion of it flowing through the Caribbean Sea into the Gulf of Mexico, and thence through the Gulf of Florida. If we estimate the width of this part of the stream to occupy in the narrows thirty-two miles out of a total breadth of forty-two miles, and its depth at 200 fathoms, its velocity at an average rate of four miles per hour, *i.e.*, in the narrows, it is equal to a stream 2,650 miles wide, 60 feet deep, and running at the rate of one mile per hour, which shows that it is not the mere rivulet it is sometimes described to be. I am aware that its average rate is now said to be less than four miles per hour; but I myself travelled through the Gulf of Florida twice a month for two years, once a month when bound to the northward, keeping in the strength of the stream, and I cannot help thinking that its strength is now very much undervalued, probably in consequence of its rate, as noted, not being strictly confined to the narrows.

This stream is uplifted as it flows out of the northern entrance of the Gulf of Florida by the inflow beneath it of colder and heavier water flowing in the contrary direction from the Arctic Pole; and this, in my opinion, accounts for the arched form of the surface of the Gulf Stream as noted by Maury. Off Hatteras it is only 100 fathoms deep, and being beyond the influence of the trade winds, its easterly momentum, due to its northerly flow, inclines its course to the northward and eastward. When it gets to the northward of 40° of lat., which it does in about 50° of W. long., it appears to spread itself out over the ocean. Now this warm stream has been giving out volumes of vapour during the whole of its northward course, and has from this cause been gradually getting saltier and saltier; and as it gets shallower, this effect must naturally be greatly increased, besides which the temperature of the stream begins rapidly to decrease. It, in my opinion, then flows onwards towards the pole, gradually losing temperature until it meets with Polar water, which, though colder, has, owing to its admixture with glacial water, a less specific gravity than itself; it then dips below the surface, and, getting colder and colder, runs with great rapidity to the bottom of the Polar basin.

I must now try and prove that this is what takes place, and for this purpose I shall quote from Maury and Capt. Nares.

Maury, vol. ii. pp. 184 and 185.—“Capt. Duncan says, Dec. 18, 1826:—

“It was awful to behold the immense icebergs working away to the north-east from us, and not one drop of water to be seen; they were working themselves right through the middle of the ice.

“Feb. 23, lat. $68^{\circ} 37'$ N. long., about 63° W., about 3 P.M., the iceberg came into contact with our floe, and in less than one minute it broke the ice. Again he says, the berg was drifting at the rate of about four knots, and by its force on the mass of ice was pushing the ship before it, as it appeared, to inevitable destruction.

“Passed Midshipman S. P. Griffin, who commanded the brig

Rescue in the American searching expedition after Sir John Franklin, informs me (*i.e.*, Maury) that on one occasion the two vessels were endeavouring to warp up to the northward in or near Wellington Channel, against a strong surface-current, which of course was setting to the south; and that whilst so engaged, an iceberg with its top many feet above the water came drifting up from the south, and passed by them like a shot, although they were stemming a surface-current both against the berg and themselves. Such was the force and velocity of the under-current, that it carried the berg to the northward faster than the crew could warp the vessel against a surface- but counter-current.”

Capt. Nares, in the Report of the *Challenger*, No. 2, says:—

“All the observations, however, agree in denoting that at a depth of from 80 to 200 fathoms there is a stratum of cold water lying intermediate between the superheated surface-water and the warm underlying layer, which is evidently the continuation towards the cold regions of the main oceanic flow of water.”

If Capt. Nares had continued his investigations to the southward of $65^{\circ} 42'$ S., and it had been possible to trace this warm layer as it gradually decreased its temperature, I have no doubt that its course might be traced to the bottom.

I could adduce further confirmation of these views if space would allow me. If my readers will look at a globe, they will readily see that the Arctic Ocean is comparatively a very small sea, and that the effect of large volumes of salt water pouring into the bottom of the Polar basin must elevate the lighter, because fresher, surface-water, and consequently cause a constant outflow towards the equator. A very large stream, known as the Labrador current, runs off as a surface-current through Davis Strait, one fork dipping below the surface, at some seasons of the year, as far to the southward as 42° N., and then underunning the Gulf Stream; and the other fork, running over the tail of the great bank, and flowing in-shore of the Gulf Stream, runs along the American coast as far south as Florida. The velocity and boundaries of all these streams vary greatly at different seasons, that is to say, the position of the sun affects the ocean as much as it does the atmospheric currents.

A further argument in favour of the cold currents flowing from the surface at the poles is that this is exactly what happens in the circulation of the atmosphere. The north-east and south-east trades, which are generally admitted to be Polar currents, descend on the equatorial side of 30° of lat.; besides in no other way that I can see can you obtain a sufficient motive power. A *primum mobile* depending on the lateral pressure of a column of

Polar water as opposed to the lesser weight of a column of temperate or of equatorial water, assuming the length of the ocean (counting say from 70° of lat. to the equator) to be 4,200 miles, and its mean depth to be 3 miles, *i.e.*, a length of 1,400 times its depth, appears to me to be a very insufficient power to move the volumes of water which we know to be constantly circulating between the equator and the poles. (An ordinary sheet of note paper has a length equal only to 928 times its depth.) If, however, it is allowed that the cold streams flow from the surface, and that they do not dip till they reach 70° of lat.¹ (the Labrador current, as before stated, dips much further towards the equator, sometimes in 42° N.), you have still a fall of nearly 3 feet 7 inches per mile of lat. to the equator.² I have said sufficient to indicate very briefly my opinions. The arguments I have recently read on this subject appear to be based on the idea that currents flow in one lateral sheet from the pole to the equator. If they did this, there would be no reason why the surface-currents should not flow in a similar way. But they do not; and, if of the depth lately suggested, *i.e.*, 3,000 feet, the Arctic basin could not receive them if they did. The *Challenger* observations seem to me to entirely disprove this view of the subject.

There is a wonderful similarity between oceanic and atmospheric circulation, which I propose more specially to point out at some future time. If we wish to know how the N.E. trades and the S.W. winds pass one another in the upper regions of the atmosphere, let us question the currents of the ocean, and the Labrador current will suggest an intelligible reply. If, on the other hand, we want to know what is the system of ocean circulation, let us ask the currents of the atmosphere; and the Polar currents (*i.e.*, the trade-winds) and the equatorial currents (*i.e.*, the westerly winds of the temperate zones) will strongly suggest to us the answer. It is quite true that there is no salt in the atmosphere, but there is, instead, vapour, which plays as important a

¹ They at most, if not at all, seasons dip in a much lower latitude.

² Estimating the depth of the ocean as 2,500.

part in its circulation as salt does in that of the ocean. In conclusion, I would say, look at the isotherms between $65^{\circ} 42' S.$, and $50^{\circ} 1' S.$ published in Report No. 2 of the *Challenger*.
May 10 DIGBY MURRAY

ANCIENT GLACIERS IN AUVERGNE

HAVING just returned from Auvergne, where I have been searching for the tracks of former glaciers among the old volcanoes of the Monts Dome and Mont Dore, I send a few notes to NATURE, in the hope that they may prove useful to other geologists who may explore that most remarkable and interesting country during the ensuing summer. My companions were three members of the Cotteswold Naturalist's Field Club, Sir W. V. Guise (the President), Sir David Wedderburn, and Mr. Lucy, all well versed in the phenomena presented by glaciation.

With regard to the Monts Dome and the country round Clermont Ferrand, it is evident that no glaciers have occupied the vales since the outpouring of the later lava currents, and the volcanic outbursts of the craters of the Puys de Dome; and yet, as I have already mentioned in the pages of NATURE, M. Le Coq discovered remains of the Mammoth, Tichorhine rhinoceros, and *Spermophilus*, which had been washed into drifts and fissures in the most recent lava currents of Volvic and Gravenoire near Beaumont. Such drifts deserve *especial attention*, as they appear to owe their origin to a period when there was greater transportation of angular and subangular débris by rain-wash and melting snow, or neve, than there is at present. It may have been during this period that the northern animals became inhabitants of Central France. Such angular and sub-angular drifts may be seen in various localities as in the road which descends from the south side of Gergovia, between the village of Merdogne and the high road to Clermont Ferrand, and again at the base of the Puy Dallet, where the high road descends to the village of Dallet, and atmospheric drifts are seen to overlie the old river shingle of an ancient Allier. The geologist who examines the source of the old basaltic current which Mr. Scrope believes to have flowed from the Puy de Berzé, near St. Genest de Champanelle, and to have extended over the freshwater strata of Gergovia, may learn a good lesson as regards the deceitful appearances of glaciation often set up by granitic rocks. Most of the country between Ceyrat, near Mont Rognon, and Theix looks regularly "moutonné," and may mislead anyone who has not become convinced, by careful examination, that this appearance is owing to atmospheric weathering, and the desquamation of the granitic rocks which separate at the joints and weather into rounded boulders assuming sometimes the aspect of blocs perchés. There are no signs of glaciation, however, among the older basalts which overlie the granite rocks, in so many localities, and which ought to show it if glaciation there had been.

In the country of the Monts Dore the evidence is most puzzling, and in some respects contradictory. Arrived at Monts Dore des Bains we searched carefully for glacier evidences in the valley of the Dordogne and the gorges de l'Enfer and de la Cour, and though some of the knolls are rounded, and there is a vast amount of débris from the rocks around and above, nowhere could we see signs of true moraines, perched blocks, or the usual evidences of glacier action; and certainly the position of the masses of rock called "Les Trois Diables," which I believe are by some set down as blocs perchés, are far too close to the rocks *in situ* to allow us to attribute their transportation to a glacier rather than to a fall from the precipices above. They belong to the "Chemins du Diable," which are preparing for a similar descent. Again, and I must here state that I arrived at conclusions contrary to those of my friends, I believe that a glacier has descended, in long ago ages, down the valley of the Dordogne, but so long since that the vast masses

of débris which have fallen from the rocks which skirt the valley, combined with the wear and tear of atmospheric agencies, the constant shifting of the bed of the Dordogne and its hundreds of tributary rills which during the melting of the snows everywhere wash, roll, wear, and transport the débris of the vale, all have assisted in destroying and masking any glacier evidence there may have been in past times. I was led to this conclusion from the examination of the higher ground, and the detection of what I believe to be moraine matter and transported rock masses, on the road between Mont Dore des Bains and Latour, as on the platform below the Rochers de Beauzac, &c.

The Tranteine valley, where Dr. Hooker discovered the transported rock-masses and which he has already described in NATURE, lies at right angles to the Dordogne valley, runs due south, and faces the Cantal. It is difficult to understand why glacier relics should be preserved in this valley and none in that of the Dordogne. This difficulty, however, vanishes somewhat when surveying the difference in the contour of the ground, the difference in the watershed of streamlets, and the low hill against which the great rock-masses are stranded, consisting of moraine matter overlying beds of basaltic lava. The Tranteine valley may be reached by passing over the Col between the Pic de Sancy and Puy Ferrand, and turning down the gorge to the south, or by the long roundabout route through the village of Latour. We selected the former for our first attack, taking the Latour route two days later. I would here recommend as guide, Guillaume Pierre, of the Hotel Chabourg aîné, to whom I pointed out certain phenomena on descending the gorge, which I think are worthy of notice. I also recommend no one to attempt this route who is not a good walker—"Facilis descensus," &c. The transported rocks, one of which Dr. Hooker sketched, lie stranded in moraine matter, which again rests on beds of black basalt, as may be seen at the little waterfall of the Tranteine stream. The rocks themselves come from the Pic de Sancy, and consist of what Scrope calls "porphyritic trachyte," but perhaps now they may be termed a granitic felstone or a felstone porphyry. Dr. Hooker calls them domite, but this term is now usually applied to the white, light, pulverulent rock like that of the Puy Sarcoui in the Puys de Dome. On the right and left of the transported rocks the hills are rounded, and blocs perchés are seen resting on them. There is a fine section on the Tranteine stream, en route to Picherande, where large transported rock-masses may be seen resting on glacial till. Following the valley down to the bridge which crosses the Tranteine river between Latour and Picherande, the observer will find rounded surfaces and transported moraine matter, but a vast deal of atmospheric weathering has gone on since the days when the ice passed away.

Travelling down the valley of Besse to Lake Pavin, I thought I recognised glacier action; and again at the head of the valley of Chambon; but if glaciers ever flowed down these valleys, it is evident that they must have done so before the eruption of the Puy de Tartaret or the Puy d'Eraignes. The occurrence of the volcanic cone of Tartaret right in the middle of the valley of Chambon is fatal to the supposition that a glacier of any size ever came down from the mountains since the outburst of the cinders and lavas of Tartaret.

If, therefore, after three visits to the volcanic regions of Central France I may be permitted to give a broad view as to the time when glaciers swept down the valleys of Mont Dore, I should say that it was in days of old, when the Alpine glaciers reached the Jura, and the Rhine glacier swept over to the plains of Bavaria, when there were glaciers in the Vosges and in the Black Forest; and that when those ice rivers melted and passed away, so also did the glaciers of Mont Dore.

W. S. SYMONDS

Pendock Rectory, Tewkesbury, June 5